Review of Physics, Philosophy and Theology: A Common Quest for Understanding
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This volume comprises eighteen papers presented at a Study Week held at the Papal Summer Residence at Castel Gandolfo in September 1987 at the direct instigation of Pope John Paul II, designed to mark the tercentenary of the publication of Newton's Principia with a discussion of the meeting ground between the theological and scientific cultures.

The papers are grouped under three headings: Historical and Contemporary Relations in Science and Religion; Epistemology and Methodology; and Contemporary Physics and Cosmology in Philosophical and Theological Perspective. The participants included most of the leading protagonists in this general area. Scentifically minded theologians included Ian Barbour, Ted Peters and Robert Russell, to mention just a few names. Then there were philosophers such as Mary Hesse, Ernan McMullin and John Leslie, and finally physicists and cosmologists, most notably Frank Tipler, Chris Isham and John Polkinghorne (who is now, of course, himself an ordained priest and Dean of Trinity Hall, Cambridge).

Some other participants in the Conference including Arthur Peacocke and Charles Misner did not contribute their papers to these proceedings.

So what overall impression does one get from reading these very diverse papers? Firstly, that everyone is trying to be very reasonable, to anticipate objections, to be tentative in drawing strong conclusions, to heal over ancient disagreements. No-one wants to see a 'God-of-the-gaps', filling in with theology, where the scientific story does

not seem to be complete. There seems to be a consensus that science and theology deal with different issues, very, very crudely the division between fact and value. There is much emphasis that theology is concerned with moral questions, the 'meaning of life', and so on. But, of course, theology does make factual claims, like the Resurrection of Christ. This may have all sorts of moral implications, but these seem to depend for their validity on the factual truths involved. But for the modern theologian it appears that such claims are not to be understood literally, but only in some metaphorical sense. There is much play with the fact that science also deals with analogies and metaphors. Theological revelation is attuned to what people can understand in terms of their own culture. No-one seems to take Biblical Fundamentalism seriously. The Southern Baptist is not regarded as any sort of bona fide theologian.

The general approach of demythologizing religious teaching seems to make the theologian invulnerable to any sort of rational critique along the lines of David Hume's critique of Miracles. We are back to Revelation, hermeneutically interpreted, with some allowance for religious experience, and the whole underpinned by commitment through Faith, which the scientific unbeliever would no doubt see as 'belief without evidence'.

But no-one in these pages is in the business of trying to debunk religion, of dismissing it as a product of wishful thinking, of self-deception, as comprising essentially mere superstition. it is rather a pity that the gloves are never off, with the theologians given their chance to reply to really trenchant criticism, perhaps even to rubbish science with its materialistic preoccupations, the limited nature of its 'explanations', the ideological components in its methodology and so on.

This book is the exact opposite of all that - sweet accord, a joining together of complementary endeavours, certainly nothing vitriolic, nor even really tendentious.

The issues of most relevance to the scientist are those concerned with modern cosmological theories of creation, and the interpretation of quantum mechanics. On the first topic, Chris Isham provides what, in my view, is the best paper in the whole collection. Very clear and technically accurate in its discussion of the Hartle-Hawking model, Isham gives just the right emphasis to the conceptual novelties involved, but never oversteps the mark of the legitimate relevance of such studies to theology. Similarly John Polkinghorne's excellent paper on the Quantum World is nicely judged, and refreshingly free from the extravagant flights of fancy that often arise in this connection! The paper on the same topic by Robert Russell shows, I believe, the dangers. Despite all the professions of a tentative exploration, Russell allows himself some wild speculations about the relevance of quantum correlations to the union of Man and God, and even the ecumenical movement amongst the churches!

In summary this book is very well-intentioned, a very sincere attempt at a dialogue across the disciplines. Whether it succeeds, each reader must judge for himself, from his own particular vantage point.

Review of P. Galison: How Experiments End
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Philosophers of science have traditionally been more interested in theory than experiment. Experiment has often been identified with mere observation, and even assimilated to the philosophical problems associated with perception. The traditional schema has seen theories as issuing, when associated with appropriate initial and boundary conditions, in the prediction of 'pointer readings'. Two problems have then presented themselves: how, from private, subjective, sense experience, can we know the public, objective fact of the pointer reading? And, secondly, given that we do know of the pointer readings, how does that bear as evidence on the truth or falsity of the theory?

The first person to appreciate fully the gross over-simplification of the above schema was undoubtedly Pierre Duhem who saw clearly that the 'practical fact' of the pointer moving over a scale was not what scientists worried about when they did experiments. They were concerned with whether they could correctly interpret the 'practical fact', as evidence for a much more subtle 'theoretical fact', what Poincaré referred to as a 'scientific fact', the occurrence of a highly interpreted and essentially theoretical phenomenon'. This 'phenomenon' was itself linked via tenuous auxilliary assumptions with the central ideas of the high-level theory about the nature of matter, light or whatever in question.

The theory-ladenness of observation has by now become a commonplace, but in the view of some philosophers such as Kuhn and Feyerabend, has led to the view that the practice

practice of science, characterised as theory under the control of experiment, is a quite irrational enterprise. Modern sociologists of science, such as the so-called Strong Programme of the Edinburgh School, have claimed that scientific knowledge is no more than a 'social construction', without any objective significance. The hard 'facts' of experimental science have crumbled totally away and become the mere signposts of a social practice.

In his very timely new book How Experiments End, Galison has looked in impressive detail at three very important experimental programmes, and proceeds to relate the historical record to a number of significant methodological questions.

The three investigations in question comprise the gyromagnetic effects, in the period 1915 to 1920, the discovery of the muon in the late 1930s, and the elucidation of neutral currents in weak interaction physics in the CERN and National Acceleration Laboratory (now known as Fermilab) experiments, whose results were published in 1973 and 1974 respectively. The three topics are well chosen since they illustrate very well what actually goes on in experimental physics, and how the practice of experimental physics, as a social phenomenon, has changed from the relatively inexpensive table-top apparatus at the beginning of the century, to the massively funded collaborative effort of dozens, even hundreds, of physicists, that characterise modern high-energy physics.

Before we discuss Galison's conclusions, with their relevance to the philosophy of science, I will briefly remind the reader of some of the scientific issues involved in the examples.

The gyromagnetic effects relate to the connection between magnatism and mechanical rotation. There are two converse

effects, the Einstein-de Haas effect refers to the tendency of an iron rod to rotate when it is magnetized, the Barnett effect to the magnetization of an iron rod which is set into rotation.

Theoretical expectations in this area go back to Ampère's 'molecular current' theory of magnetism, and Maxwell himself carried out unsuccessful experiments to test for gyromagnetic effects. But one thing that Maxwell lacked was a calculation to show the magnitude of the effect to be expected. This was provided by Lorentz's electron theory, and in 1908 Richardson showed that, for an electron in an atomic orbit, the ratio of mechanical angular momentum to magnetic moment would be just 2 m/e, where m/e is the mass-charge ratio of the electron. If an iron bar were magnetized it would be set into rotation by conservation of angular momentum. But the effect is very small and Richardson's attempt to demonstrate it failed. De Haas and Einstein, in 1915, were the first to succeed, by reversing the magnetization in a cyclical fashion and exploiting the resonance with the rotational oscillation of the rod. The converse effect was also demonstrated in 1915 by Barnett, who had been led to the investigation from the mistaken idea that the earth's magnetic field might be a direct consequence of its rotation (in fact the Barnett effect contributes less than 1 ten-billionth of the earth's magnetic fied). Further investigation of gyromagnetic effects were undertaken by Stewart in America, by Beck in Switzerland and by Arvidsson in Switzerland. The results could be expressed in terms of a g-factor measuring the ratio of the actual magnetic moment to that predicted for orbital motion by Richardson's simple formula. Einstein and de Haas confirmed the 'theoretical' prediction that g = 1, but the later experiments settled on a value close to 2. It was not, of course, until Dirac's relativistic electron theory of 1927, that high-level theory would predict a g-factor of 2 for the electron. In this case then the experimental value came decisively first,

and there was no sense in which the gyromagnetic experiments were ended, when theory agreed with the expected value!

The muon story illustrates nicely some rather different points. Ever since Hess's balloon-born experiments in 1912 it had been clear that cosmic rays entered the atmosphere from outer space. But what was the nature of the penetrating component that could reach deep inside a mine or to the bottom of a lake? Could they be high-energy electrons, that interacted much less strongly than theory predicted, with the atmospheric molecules? But to test that idea needed an application of high-level theory to predict exactly how high-energy electrons would behave in the atmosphere. This was not provided until the work of Carlson and Oppenheimer in 1936, who showed that quantum electrodynamics could account for all the features of cosmic ray showers, except the residual penetrating component, which must comprise some new weekly interacting particle. In 1937 Street and Stevenson obtained a famous cloud-chamber photograph showing a muon near the end of its trajectory, where it was travelling sufficiently slowly for its mass to be measured. A figure of about 130 electron masses was obtained. The irony of the whole story is that the 'mesotron' as it was then called, was thought to be related in some way to Yukawa's theoretical particle, responsible for the strong interactions between nucleons. But that was not the muon at all, but the pion, discovered by Powell and his collaborators in Bristol in 1947!

Again we have an example of a very important part of the experimental art, disentangling an effect from 'background', so that it stands out clearly on its own.

The final episode discussed by Galison is the neutral current story. This is the longest, and in many ways, the most interesting, part of the book. The original theory of

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the weak interactions, due to Fermi, involved a point interaction between charged leptonic and hadronic currents. It was long suspected that this interaction was mediated by massive charged bosons, the W-particles. There was also the possibility of neutral current interactions mediated by a neutral Z- particle. By the late 1960s few people believed in neutral currents, since they would allow strangeness-changing decays such as kaon -> pion + neutrino-anti neutrino pair, which were never observed experimentally.

However, on the theoretical front, a unification of weak and electromagnetic interactions had been proposed in 1967 by Weinberg and Salam, which did involve a neutral Z, the massive counterpart of the familiar photon. But it was only in 1971 that the new electroweak theory was shown by Gerardus t'Hooft to be renormalizable, so that it could be used to make sensible predictions. But there was also the problem of the non-occurrence of the strangeness-changing neutral current processes referred to above. The solution to that theoretical problem only came with the postulation of a fourth quark as constituent of hadrons and the demonstration by Glashow, Iliopoulos and Maiani, that a mechanism could be devised within a four-quark framework of suppressing the unwanted strangeness-changing neutral currents. The GIM mechanism was actually proposed in 1970, but nobody paid much attention to it until the discovery of the fourth charmed quark independently by Richter and by Ting in 1974.

So by hindsight physicists were left with the possibility, or more strongly the prediction, of non-strangeness-changing neutral currents events, paradigmatically the scattering of a neutrino by a proton. It was the demonstration that muon neutrinos could scatter inelastically but without themselves changing into muons that constituted the experimental discovery of neutral currents in 1973/74.

But that simple story, a theoretical prediction being confirmed, is quite inappropriate to the detailed history. The CERN experiment was proposed ten years earlier, to exploit the building of the proposed new giant bubble chamber, named Gargamelle, in a search for the massive charged W-particles, thought to have masses of a few GeV. The neutrino-induced events that would have signalled neutral currents could well have been due to neutrons.

The W-particles were never revealed. (Their masses were a hundred times larger than the CERN physicists had originally thought, and they were only demonstrated together with the Z, using appropriately higher beam energies, in 1983.) But there were plenty of muonless stars being produced in the bubble chamber. But how could the neutron background be excluded? Only by subtracting it, and that meant calculating it, and it was the reliability of these calculations which were the main issue in interpreting the experiments at CERN, and the independent EHA collaboration at Fermilab, which used a quite different electronic type of detector, the spark chamber.

The whole story is told in very impressive detail by Galison, who has researched a huge collection of project proposals, conference proceedings, scanning records, administrative correspondence, and even what he describes as the 'archeological remnants of the equipment stacked in storage areas'!

So what are the conclusions to be drawn from these case-studies?

Firstly that experimental science has a certain autonomy, in relation to high-level theory. progress depends very much on questions of technological feasibility, on developments on instrumentation and so on. Modern high-energy experiments take a long time to plan, involve an enormous investment in terms of money and manpower, and are somewhat inflexible

towards changing the fundamental operation, once it is set in train.

But having said that experimentalists do not work 'in the dark', just noting pointer readings. They have to have theoretical notions of what is going on! But the theory is usually of a phenomenological character, enabling estimates to be made of reaction rates, branching ratios and so forth. This is of vital importance in dealing with the pervasive problem of 'background', which is really an application of Mill's Method of Residues, subtracting the effects of known causes, to isolate the effect of the unknown cause. This is part of the more general problem of experimental error. Systematic errors in experiment always arise from not making allowance for all the factors that make relevant contributions to producing the publicly agreed 'practical fact'. The practical fact produces a number, but does it measure what it purports to measure? That is the basic problem that faces the experimentalist.

So how do experiments end? Essentially when Duhemian 'good sense' intervenes and everyone agrees, perhaps for different reasons, that all the relevant sources of error have been identified and adequately allowed for, so that a clear 'effect' is identified, available for further investigation. Of course none of this is logically compelling - there are always loop-holes, there is always more to be said in trying to persuade the persistent sceptic, but that does not mean that we should be cynical, assume that all experiments are 'cooked' (which is not of course to deny that some are!), that experiments end when we get the result we want, that the gyromagnetic effects, the muon and the neutral currents are mere 'social constructions'.

In fact, experimental scientists do, in the main, have open minds - they allow themselves to be persuaded, to be convinced, even against their own interest, if that is how the experiments turn out.

Experimental science is essentially a rational activity, not one that can be reduced to logic-chopping rules, but certainly not allowing that 'anything goes'. Galison has done a splendid service in not just stating these obvious truths, but in arguing for them, with all the scholarly historical detail needed to rescue experimental science from the grip of the sociologists and the irrationalists. He is to be congratulated on producing a masterpiece in the field.